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EDITORIAL.

THE success of the recent meeting of the Geological Society of America was undoubtedly due to the fact that it was held in Washington. No other city in the country offers so many attractions to geologists in the winter time as the national capital. Containing as it does the largest body of geological investigators to be found in any one place in the world, it has become a center of geological activity and the repository of many valuable collections. Located within easy reach of the universities of the East and South and of the Middle West it has become a favorite rendezvous for geologists scattered throughout these parts of the country. For these reasons the suggestion made by Mr. Walcott, Director of the United States Geological Survey, that the society hold all its winter meetings in Washington and its summer meetings elsewhere, is an excellent one, and was heartily endorsed by the retiring president, Professor Le Conte. Since the present method of entertaining the society would prove a burden upon the Washington members if repeated annually, it would be proper to allow the visiting members to share the expense by dividing the cost *per capita*. It is to be hoped that the proposition to meet annually in Washington be considered seriously by the society.

The necessity of more expeditious methods of conducting the presentation of papers and the need of a livelier sense of obligation to fellow-members on the part of those misusing the time of the meeting were apparent. It ought not to require long reflection to convince one that beyond a reasonable use of time the tax on the patience of an audience defeats the object of an address, and is more than a waste of energy.

It is to be noted that the present unsatisfactory process of conducting the annual election resulted in the only regrettable incident connected with this otherwise highly successful meeting. The present method should be changed speedily. It has been suggested that a process should be adopted by which the secretary should send to every member blanks for nominations for each officer; each member to nominate one person for each office. Upon receiving these nominations the secretary should ascertain those having the two highest numbers of nominating votes, and mail to each member a ballot placing these in nomination, and requesting a vote.

The extent of the interest taken in the meeting is shown by the number of papers presented, which reached forty-nine, and by the fact that discussions were quite generally participated in. The presence of ladies at the banquet and the efforts of the committee on entertainment and those of the toastmaster, Professor Emerson, combined to render this feature of the meeting most enjoyable. J. P. I.

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IN the October-November number of the JOURNAL OF GEOLOGY (p. 811) Mr. J. B. Tyrrell's paper on the "Genesis of Lake Agassiz" includes incidental reference to the application of the name "Laurentide Glacier," upon which it may be useful to make some comment. When it had been shown by the writer that the Cordilleran ice-sheet of the West was self-contained, it became evident that the great eastern division of what had previously been referred to as a "continental glacier" required some distinctive appellation. The name Laurentide glacier was then proposed in the following terms: "Recognizing, however, the essential separateness of the western and eastern confluent ice-masses, and the fact that it is no longer appropriate to designate one of these the 'Continental glacier' the writer ventures to propose that the eastern *mer de glace* may appropriately be named the *Laurentide glacier* while its western fellow is known as the *Cordilleran glacier*."[1] Thus it appears that the term Laurentide glacier was intended to designate an ice-sheet developed upon the entire Laurentian plateau or protaxis—the Canadian shield of Suess—which extends itself, as a relatively elevated tract of U-shaped form around the depression of Hudson Bay, from Labrador on the east to the Arctic Ocean near the mouth of the Mackenzie on the west.

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[1] American Geologist, Sept. 1890. See also Trans. Royal Soc., Canada, Vol. VIII, Sec. IV, p. 56.

Mr. Tyrrell's investigations relate particularly to that arm of the protaxis which lies to the west of Hudson Bay, and upon the northern part of this he has been able to define a center of dispersion of glacier-ice to which he has applied the name "Keewatin glacier." He suggests that the name Laurentide glacier may now be relegated to the similar center of radiation of ice shown by Mr. A. P. Low to have existed on the Labrador peninsula, and supposes that ice from the latter gathering-ground (at a date subsequent to that of the greatest spread of the Keewatin glacier) crossed the southern part of Hudson's Bay to the Winnipeg basin as well as to that part of the continent in the vicinity of the Great Lakes. It is, however, tacitly assumed that no general occupation of the Laurentian plateau by glacier-ice occurred at a period antecedent to that in which the Keewatin glacier became defined; although, to the writer, it seems probable that at one time this plateau or axis was thus covered by ice in all its length and that this ice moved down from higher to lower levels in conformity with the normal slope in all directions.

A second assumption (to which, however, several writers on this subject besides Mr. Tyrrell have given credence) is that implied in the suggested passage of glacier-ice originating in the Labrador region across the watershed to the north of the Great Lakes. The actual evidence on this point appears to the writer to be slight and inconclusive in its nature. Our knowledge of the rocks of the great region involved is, it is believed, insufficient to prove that any erratics from the basin of Hudson Bay have actually crossed this watershed to the south or southwest. Nor can the observed directions of striation be taken as definitely indicating ice-movement in a southerly sense in all cases, for at the time of observation of many of these, it cannot be doubted that a southerly rather than a northerly motion was naturally assumed, in consequence of hypotheses then unquestioned, and without the critical examination called for in view of later discoveries. So great an extension of an ice-sheet originating on the Labrador peninsula, is all the more doubtful in the light of Mr. Chalmer's investigations, which show that no glacier-ice from the northward crossed the highlands to the south of the St. Lawrence below Quebec. These highlands developed in New Brunswick and northern Maine, an independent center of dispersion, which Mr. Chalmers suggests may be referred to as the Appalachian system of glaciers or the Appalachian glacier.[1] If, therefore, we may admit the existence of a Laurentide glacier of early date and such as to envelop the whole Laurentian plateau, the Keewatin glacier of Mr. Tyrrell, with that which may be named the Labradorian glacier,[2] as known to us by Mr. Low's work, may be regarded as relatively local (although still very important) centers of dispersion connected with a diminishing stage of the glacial period. In this case the overriding of the area at one time covered by the southward extension of the Keewatin glacier by ice from the eastward (as demonstrated by Mr. Tyrrell) may be attributed merely to a still later reëxtension, due to climatic changes, of that part of the Laurentide glacier situated to the east of Lake Winnipeg. If, on the other hand, it should ultimately be shown that no continuous ice-sheet ever covered the entire length of the Laurentian plateau, the several great glacier masses actually known to have been developed upon these highlands may still be appropriately considered as constituting together a Laurentide group, as distinguished from the great western Cordilleran ice-mass.

In the case of the Keewatin and in that of the Labradorean glacier, it has been shown by Messrs. Tyrrell and Low that the centers of dispersion changed, giving rise to superposed series of striations, and it appears probable that in the study of this

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[1] American Geologist, Vol. VI (1890), p. 325.

[2] This has, I believe, already been proposed by Mr. F. B. Taylor.

migration of gathering-grounds of glacier-ice on the Laurentian highlands, the explanation of many otherwise anomalous circumstances will yet be found. Any inquiry into the causes of such migration opens a wide field of discussion, including particularly meteorological and possibly changed orographical conditions. Other things being equal, it is obvious that the places in which the accumulation of glacier-ice began would also in all probability be those where the last centers of its dispersion continued longest and ultimately failed. It is very important to trace the migration of these centers from which the ice flowed, and if possible to ascertain the conditions which, in all probability, may have interfered with the coincidence of the initial radiant areas and those marking the close of the epoch of glaciation.

G. M. D.

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IT would appear from the papers of Professors Tarr and Barton read at the recent meeting of the Geological Society at Washington, abstracts of which appear in this number of the JOURNAL, that Professor Salisbury and myself must have invited misinterpretation of our views respecting the former extension of the inland ice-sheet of Greenland by infelicities of expression not altogether evident to us. What we have thus far published has consisted, in the main, of brief statements intended rather to show the bearing of the topography of the coast upon the larger questions of glacial prevalence than to indicate the precise local extension of the ice. In the series of papers entitled "*Glacial Studies in Greenland*" (not yet complete, having been interrupted to give earlier place to contributions awaiting publication), the specific subject of former glacial extension has not yet been reached. When it shall be, however, we shall not wish to be understood as attempting to map the precise limit of former glaciation on the thousand miles of borderland along which we coasted, but merely as endeavoring to indicate its general limitations. We believe, neverthless, that our observations and inferences are thoroughly trustworthy in the general sense in which they are intended to be received, and that they are decisive in their bearing upon two general hypotheses of wide interest; the one, that the ice-cap of Greenland formerly stretched across Baffins Bay and Davis Strait and became the source of the mainland ice-sheet; the other, that, in a former supposed state of elevation, the ice-cap pushed out into the heart of Baffins Bay or into the Atlantic until it reached adequate conditions of wastage by flotation or by low-level extension. In either case, the borderland of Greenland must, presumably, have been effectively glaciated for a long period, and should manifest this in the subjugation of its topographic contours. In its bearings upon these general problems, an advance of a few miles, more or less, an ineffectual overtopping of a few heights, more or less, are relatively inconsequential. Our language is to be interpreted in the light of the major questions whose solution we sought.

There does not appear, however, to be any essential lack of harmony between the data of Professors Barton and Tarr and the interpretations of Professor Salisbury and myself, unless it be in relatively unimportant details. Professor Salisbury makes the following statement relative to the region (this JOURNAL, Vol. III., pp. 876–7):

> On the whole, judging from topography alone, it seemed more probable that the coast from about latitude 70° north to the end of the Nugsuak peninsula [the southern one] had not been recently smothered in ice, though it is well possible that the ice-cap may have once extended beyond its present limits, and that isolated glaciers occupied the valleys leading down to the sea. The northwest end of the peninsula bears the marks of the passage of ice over a considerable part of the coastal front. North of Nugsuak peninsula, and from that point to the south side of Melville Bay, the topography of the coast, so far as seen, indicated general, though not universal, glaciation. Thus the southwestern end of Svarten Huk peninsula (71° 30′) has a topography denoting the absence of glaciation.

This embraces both of the districts in question.

Relative to the region of Professor Barton's studies, I made the following (unpublished) note on July 18, 1894, at the conclusion of detailed notes on Hare island, the Nugsuak peninsula, (the one south of Umanak fiord), Ubekyendt and Upernivik islands, Svarten Huk peninsula, and Disco Island:

> I infer that the inland ice once pushed out into Baffins Bay through the Waigat and through Umanak fiord and overlapped the adjacent lands, but did not overtop the highest parts of Disco, Upernivik and Ubekyendt islands and Svarten Huk peninsula. The ice border rose 2000 feet above the present sea surface, but probably not 3000 feet on the coastal line, quite certainly not 4000 or 5000 feet. I have traced on the chart a theoretical outline of the farthest ice. This of course does not include local ice on the uncovered land. It, on the contrary, presumes it.

This hypothetical line starts with the northwestern part of Disco Island and swings outside of Hare island and some distance off the extremity of Nugsuak peninsula, touches the outer side of Ubekyendt Island, and thence connects with the southern portion of Svarten Huk peninsula. The outer curve of this line is *sixty miles away from the present border of the inland ice* and embraces the entire territory of Professor Barton's special studies, as I understand them.

I have no notes on the northern Nugsuak peninsula which was the special field of Professor Tarr's studies, the Falcon having turned away from the coast somewhat south of it to attempt the "middle passage" of Melville Bay, and, failing in this, returned to the vicinity of the coast a little north of the peninsula. On the region next south I made the following note on July 19:

> With the exception of Sanderson's Hope and a few other prominences south of it, the whole region shows rounded contours, so far as seen. The small islands are well-rounded domes of rock that appear entirely bare. I could not detect any bowlders or other débris upon them. They are simple *roches moutonnées*, but not strikingly typical as such. The reduction has not completely subordinated them to glacial types. I have no doubt the ice pushed out well into the bay here. Some of the higher peaks may have remained as nunataks.

These small islands are rather farther away from the edge of the inland ice than the similar ones described by Professor Tarr.

It appears, therefore, that we explicity recognized predominant glaciation reaching out from 30 to 60 miles, and trespassing on the borders of Baffins Bay, and that we excepted only some of the higher points. It is only by demonstrating that these were submerged by the inland ice that any notably greater extension can be established, and the most of these points were not visited by the members of the recent expedition, nor were comparable heights of like situation near the coast line studied. Sanderson's Hope is charted as 3467 feet; a point on Svarten Huk peninsula, as 5230; a point on Disco Island as 5110, and many other points range from 3000 feet upwards. At most, therefore, the differences between us are merely matters of minor detail so far as observational determinations are concerned.

This advance of 30 to 60 miles seemed to one party to call for expressions of amplitude, to the other, instinctively making comparison with the many hundreds of miles of ice invasion of the mainland, to call for diminutives. Here is, indeed, a wide *psychological* difference, and we can contribute nothing to minimize this difference. Under the widest permissible interpretation of the facts as seen by either party the advance seems to us emphatically small, and very significant in its smallness.

The reliefs of the region seemed to the earlier party to belong to the semi-subdued type. The later party appear to have supposed them to represent the *unsubdued* type of the earlier party. The determinations of the later party are, however, in close accord with the classification of the earlier party.

It is possible that Professors Tarr and Barton unconsciously transferred to this region our rather emphatic descriptions of certain markedly serrate tracts farther south, and of certain ragged islands lying to the north, *e.g.*, Dalrymple Island (figured in this JOURNAL, Vol. II, p. 661), and Cone Island (Fig. 1., Vol. III., p. 772). As indicated above, we did not class the topography of the region in question under the markedly ragged and serrate type but under an intermediate one of partial subjugation. The figures referred to illustrate our standard of the former class. The courteous suggestion that the differences of interpretation were due to our point of view on the leeside of the prominences is inapplicable to the important case of Disco Island, for Professor Salisbury passed on the stoss side. It is only measurably applicable to the rest of the territory, as the oblique views of approach and retreat gave traversing lines of vision that partially reached the stoss contours.

This rather extended note has its purpose in a desire to make clear the narrow limits of such differences of observation as may exist and to show that the two main questions of general interest are essentially unaffected by them. Glacialists may feel sure that the conclusions derived from the topographic study of a thousand miles of the coast, even though cursory, and limited by conditions, will be found decisive on the main issues.

T. C. C.